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THE AMERICAN METEOROLOGICAL JOURNAL.

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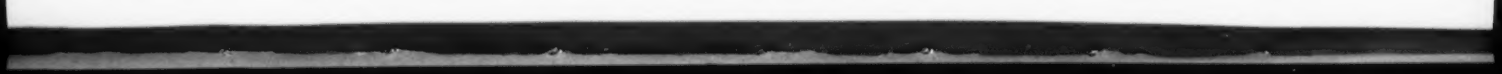


THE AMERICAN

METEOROLOGICAL JOURNAL.

VOL. IX. BOSTON, MASS., NOVEMBER, 1892. No. 7.

WIND MEASUREMENT.

W. H. DINES, F. R. MET. SOC.

THE two instruments most commonly used for the measurement of the wind are the Robinson Anemometer and the pressure plate — the former measuring the velocity and the latter the force of the wind. Although a long series of observations have been made and tabulated with both these instruments, there is still considerable uncertainty as to the real velocity of the air, both in the case of moderate winds and also during gales of exceptional severity; and it is still more or less a matter of guess-work to know the extreme pressure for which allowance must be made, when designing a bridge, or other structure, that will be exposed to the wind.

There seem to be two reasons for the state of confusion in which the science of wind measurements at present exists; the first being that the wind never blows uniformly, whereas our instruments are calibrated on the supposition that it does do so; and the second being the indifference with which the matter of exposure is treated, anemometers being often placed a few feet above the roof of a house or observatory, about the the worst position, perhaps, that could be chosen.

Prof. Marvin has pointed out the effect which the non-uniformity of the wind has upon the Robinson anemometer; and Mr. Symons in this country has often called attention to the evils of the usual method of exposure, but at present with very little result.

Probably most observers have noticed the way in which the strength of the wind varies from minute to minute, particularly

at inland stations; but few are aware of the extraordinary number and extent of the variations. The trace given by an ordinary self-recording pressure plate consists as a general rule of a blacked-in space reaching down almost to the zero line and not of a definite line. I have found that, in order to get a clear and distinct trace during an ordinary gale, it is necessary to allow at least two inches of paper to pass under the pencil in each minute; and during a storm of exceptional violence, this amount is hardly sufficient.

If it were not for these variations there would not be much difficulty in measuring the velocity accurately; but as it is, not only is the mean velocity difficult to measure, but even when it is known, it is not sufficient to show the actual character of the wind.

The two things which it is important to know are the mean pressure and the extreme pressure which may occur during the gusts. It is impossible to estimate exactly the mean pressure from the mean velocity; and the extreme pressure is only indirectly dependent upon the mean velocity. So much is this the case that the most destructive gale is not always the one in which the highest mean velocity is recorded, but the one in which the gusts are the most violent.

For a uniform velocity and with a given barometric pressure and temperature, there is no doubt a perfectly definite relation between the pressure and velocity. This relation is given by the equation $P=kv^2$, P being the pressure and v the velocity. If P be expressed in pounds per square foot, and v in miles per hour, the value of the constant k is variously stated, the extreme values being about .003 and .005. The higher value, viz., .005, appears to have been originally given in a textbook on engineering, with a footnote attached, saying that the experiments on which it rested were doubtful; and then to have been copied and recopied without the note, thus attaining an authority to which it has no claim. Recently the value .003 has been given, and since it has been obtained by several independent series of experiments, carried out by different observers, and under different circumstances, it is probably not far from the truth.

However, assuming that we know the value of k with the utmost accuracy, the relation is of little use in the case of a

continually varying quantity like the wind; for it is a well-known mathematical fact that the mean of the squares of a number of quantities is greater than the square of the arithmetical mean of the quantities. Take an ordinary wind as an example, and assume that a velocity of eighteen miles per hour corresponds to a pressure of one lb. per square foot. Suppose the mean velocity to be eighteen miles per hour, then the mean pressure will be more than one pound per square foot, but how much more it is impossible to say. The reason for this is not far to seek. When the wind drops to nine miles per hour, half the mean velocity, the pressure falls to one fourth pound per square foot. When the velocity rises to twenty-seven miles per hour, three halves of the mean, the pressure rises to two and one fourth lbs. per square foot. Now the mean of one fourth and two and one fourth is one and one fourth, which is twenty-five per cent greater than one; so that the variations tend to raise the mean pressure above the value, which for a uniform wind would correspond to the velocity.

At this place at all events the velocity of the wind is continually departing from its mean value, by more than fifty per cent; but the frequency and extent of the variations do not seem to be subject to any rule. Generally a northeast or easterly wind is much steadier than a southwest or westerly; and since this has been noticed by others I conclude that it is not a local peculiarity due to the exposure of the instruments. During last winter five different instruments were erected eighteen feet above the roof of this house for purposes of comparison; of these three were so arranged that it was possible to see the actual velocity which they were recording at the moment. From careful study of the instruments themselves, and also of their automatic records, I have come to the conclusion that a minute very seldom occurs in which the two extreme values of the velocity do not differ by at least fifty per cent, and that, as a general rule, the maximum velocity during a short period will be from two to three times the mean velocity.

The measurement of so fluctuating a quantity is not easy, and depends to some extent at least upon the instrument used. The Robinson anemometer has a decided tendency to measure the mean pressure rather than the mean velocity, and hence the effect of the variations is to make its record too high. If two

instruments, one of the cup form and one of the old Whewell windmill form, calibrated to give the same result by means of a whirling machine, be placed side by side in the open air, it will be found that they do not agree. The same thing may be noticed, but is not so marked, when two instruments, both of the cup type, but of different sizes are used. The question naturally arises, Which is correct? Again, if the same instrument be tried on a whirling machine indoors, and then be tried in the same way on a calm day out-of-doors, the two results will be different. I have tried this with several instruments, using every possible care and selecting the calmest days, but always with the same result. The instrument, if calibrated by an indoor trial, will register at least five per cent more wind than it would have done, had it been first tried out-of-doors. I am inclined to think that the outside calibration is the best, and that the discrepancy is due to the eddy set up in a closed building by the motion of the instrument. Whether indoors or out-of-doors the motion is quite uniform, and in that respect different to the natural wind. If, however, a Robinson anemometer be tried out-of-doors on a whirling machine on a day which is not calm, it will be found that the factor, after the proper correction is applied, is much lower; and as the conditions in such a case more nearly resemble those of the natural wind, there is good ground for taking them in preference to the others.

The action of the changes in the velocity upon the Robinson anemometer is not an unmixed evil, for its effect is more marked in a light than in a strong wind. It happens that for a uniform wind the factor is greater with a low than with a high velocity; and these two errors thus tend to cancel out each other, with the result that the factor is more nearly constant than it otherwise would be.

There are some respects in which it seems more desirable to measure the pressure than the velocity. Certainly the pressure is the element which is of most practical importance. There seems at present to be no instrument in use which gives a record of the mean pressure, excepting those which require a clock and leave an automatic record, and possibly this is one reason why so many more velocity anemometers are in use. There is, however, another serious objection to the use of a pressure plate. On many days the mean pressure is certainly

less than one ounce per square foot, but the instrument must be constructed to register at least thirty pounds per square foot. If the scale is of any reasonable length, one five hundredth part of it, which is about the proportion of one ounce to thirty pounds, is too small to be distinguishable. Beside this, if the instrument be sufficiently strong to withstand a gale, it is hardly possible for it to be delicate enough to measure so small a force as one ounce. The result is that a pressure plate, at all events at an inland station, on many days shows no sign of any motion.

A plate, which shows simply the maximum pressure since it was last set, is very easily constructed, and is not open to either of the above objections. It is a matter of regret that so few of such instruments are in use.

A pressure plate, unlike a velocity anemometer, is very easily calibrated, and it is therefore strange that so much doubt should have been thrown upon the high pressures recorded by these instruments. Thirty pounds per square foot is a not uncommon amount, and pressures exceeding sixty pounds have been recorded. There is little doubt that few buildings could withstand these pressures if they were fairly applied to the whole of one side, and hence some explanation of the fact that the buildings do stand is required. Various reasons have been given to account for these high pressures. It has been suggested that the area over which they act is very small; that the pressure upon a large area is proportionally less than upon a small one; also that the observed amounts are due to defects of the instruments and do not really occur. The second theory is true, and may be proved so by any one who will take the trouble to try a simple experiment. If two plates of similar shape but of different areas be placed at the ends of a lever and then exposed broadside to the wind, it will be found that the fulcrum about which the pressures on the plates maintain equilibrium is nearer to the smaller plate than might be expected. Thus if one plate were four times as large as the other, and the whole pressure were proportional to the size of the plate, the fulcrum should be at one fifth of the whole distance between the centres, from the centre of the smaller plate. It will be found at once, on trial, that this position does not give equilibrium, but that it must be shifted considerably nearer to the smaller plate. It is,

of course, necessary, in trying this experiment, to see that the plates are a fair distance apart, so that each one may be free from the disturbing action of the other, and also to be careful that the lever itself offers no appreciable resistance to the wind. A common board with the edges feathered answers very well for a lever; and in practice it is more convenient to have the large plate and the fulcrum fixed in position, and to find the position of equilibrium by sliding the small plate along the lever, as is done in the case of an ordinary steelyard.

The question as to how far the exceptionally high pressures are due to defects in the instruments is not easy to answer. It seems quite probable that the momentum of the moving parts carries the index beyond its proper position when the motion of the plate is at all violent. Mr. Ellis has stated that the Osler Anemometer at Greenwich Observatory used to record very high pressures; but that in 1882 he suggested that certain alterations should be made in the mechanism by which the motion of the plate was communicated to the pencil, and that since that has been done, no exceptionally high pressures have been recorded. The extreme force of a gust of wind often lasts for only a short time, less than a second certainly, and the actual pressure registered will depend on the inertia of the moving parts of the instrument. It would appear, however, as though the registered amount would be too low rather than too high, since a small force acting for a long time is in many respects equivalent to a large force acting for a short time; and consequently the shortness of the time, during which the extreme pressure lasts, should not allow its full effect to be felt. It is very undesirable to have the pen or pencil at any distance from the plate, for if so it is hardly possible to have the connection between the two so rigid and non-elastic that we may feel certain that the pencil exactly reproduces the motion of the plate. It appears to me that the tube type of anemometer is the only one in which the registering apparatus may be safely placed away from the part exposed to the wind; for, apart from the use of electricity, it is the only kind in which the motion or pressure can be transmitted to a distance without loss by friction, or appreciable momentum in the parts which form the connection.

The question of the exposure is undoubtedly a very important one. The prevailing idea seems to have been that, so long as

there was nothing in front of an anemometer, an obstruction behind or on one side did not matter. This is certainly a mistake, and the sooner it is recognized that no exposure can be satisfactory unless there is a perfectly clear space on all sides of the instrument, the better. The actual space which must be left can only be determined by experiment, but it may safely be said that the larger the obstruction to the free motion of the air, the more necessary it is to keep the instrument away from it.

The exposure is of especial importance in the case of a Robinson anemometer, for if one side of the axle be exposed to an eddy from which the other side is free, the error so caused may be very great. Incidentally, I found that at a height of nine feet above the highest part of an ordinary house, excluding the chimneys, an instrument might be affected to the extent of thirty per cent by an eddy from the house itself, but that at eighteen feet no sign of any eddy could be detected.

The top of a pole would appear to be the most suitable position, the pole being placed in an open field ; for if the position is close to any trees or buildings, there is nothing gained by using the pole, and the anemometer might as well be placed on the building. After all, the question of exposure is simply one of expense, for it is only necessary to go to a sufficient height, and there is no difficulty about fixing an instrument at an elevation that would render it free from the eddies or shelter of any tree, or of any ordinary building.

OXSHOTT, LEATHERHEAD, ENGLAND, Aug. 8, 1892.

PAPERS FROM THE LABORATORY OF PHYSICAL GEOGRAPHY OF HARVARD COLLEGE.

No. 8. — THE STORMS OF INDIA (*Continued*).

SIDNEY M. BALLOU.

THE WINTER RAINS.

THE winter storms exhibit marked differences from the transitional cyclones. Instead of originating over a warm area of water, they are formed on the land at its coldest season. They first appear in the extreme northwest of India. It has

been assumed by some that they enter India from Beluchistan or Afghanistan, but of this there is not sufficient evidence.*

Their movement, unlike that of the transitional cyclones, is eastward. They are of slight intensity and of small barometric depression, but remarkably persistent, crossing unbroken over the high hills of Northern India. For this reason they are thought to extend to a great height in the atmosphere. The northwest winds which follow them bring a spell of cold weather that we should call a cold wave. In all respects they appear to be similar to the ordinary cyclonic storms of Europe and North America.

These differences of cyclonic phenomena suggest a difference of cause for the winter storms. Their place of origin seems particularly unsuited to convectional formation. They are formed over the land and in winter, two circumstances unfavorable to convectional action. Their great height indicates that they are phenomena of the upper atmosphere, whereas convectional storms are caused by overturnings of the lower strata. Their remarkable persistency, in comparison with the amount of rainfall they cause, also suggests that there must be some cause for their maintenance in addition to the latent heat liberated by the rain.

Before the recent prominence of Dr. Hann's theory, Mr. Blanford made the attempt† to explain the origin of these storms on a convectional basis, giving as a cause for the disturbance of basic equilibrium the evaporation from the river Indus, undried swamps, irrigation, etc. It was not at all evident, however, that these causes were adequate, and after the publication of Dr. Hann's theory, Mr. Blanford readily admitted‡ the possibility of these storms being formed in the manner suggested by Dr. Hann. And, indeed, the general similarity between these storms and the ordinary storms of the temperate zone seems to warrant the conclusion that they are alike in all respects.

THE SUMMER RAINS.

The storms which occur during the prevalence of the summer monsoon, that is, from June to September, occupy in some

* Hill: Met. of Northwest Himalaya. Ind. Met. Mem., I., 404. Blanford: The Rainfall of India. Ind. Met. Mem., III., Pt. 1, 87.

† Blanford: The Rainfall of India. Ind. Met. Mem., III. Pt. 1, 92.

‡ Blanford: The Genesis of Tropical Cyclones. Nature, xliii, Nov. 27, 1890, 83.

respects a class midway between the transitional cyclones and the winter rains. They sometimes approach the intensity of the former, while in their persistent duration and ability to cross high ranges of hills they resemble the latter. The following extract* gives a general description of them: —

"During the rains proper, or from about June 15 to Sept. 15, cyclonic storms are of frequent occurrence. During each southwest monsoon season, from six to twelve storms may be expected in the north of the Bay which will be of sufficient intensity and vitality to continue unbroken after they reach land for at least twenty-four hours. Each of the storms is accompanied by squally weather at the head of the bay. Their chief feature is the strong westerly or southwesterly winds or gales which blow in the south and southeast portion of the cyclonic or storm area. These winds are frequently of force 8, 9, and 10, during the existence of the storm at sea, and occasionally of force 11.

"The average strength of the ordinary monsoon winds in the bay, it may be mentioned, is three to five. In these small storms of the rains proper, there is rarely, if ever, a well-marked centre (shown by the existence of a central calm area — the bull's eye of a storm). The depression of the barometer below its proper height in the midst of a storm area is never large. It does not often exceed three tenths of an inch, and rarely, if ever, is above five tenths of an inch. Very strong winds prevail in the south and east."

The first matter to consider is the meteorological condition of the formation of cyclones in the summer rains. This is given by Mr. Eliot as follows: †—

"The chief feature of the weather, during the rains in Northern India, is the intermittent distribution of the rainfall, or the apparently oscillatory character of the monsoon current. A fall of the barometer in Northern India is rapidly followed by and associated with an increased inrush of the monsoon current, giving a copious rainfall for several days. The barometer generally rises with the continuation of the rainfall. The current diminishes in intensity, and finally the rainfall, instead of being general in character, is reduced to occasional local showers. Usually, after a few days of this condition of affairs, the barometer again falls in Northern India, and the same succession of phenomena is again observed. . . . The great majority (if not all) of the minor storms which have occurred in the past

* Cycl. Mem., I., 1. † Ind. Met. Mem., II., Pt. 1, 1.

four years during the prevalence of the southwest monsoon, have originated during these short intervals known as *breaks* in the rains."

These storms usually originate in the northern part of the Bay of Bengal and move in a direction between north and west. As already stated, they do not disintegrate readily upon striking the land, but travel a considerable distance inland before breaking up. Sometimes they cross the entire peninsula unbroken and appear in the Arabian Sea.

One noticeable peculiarity is the unequal distribution of wind intensity about the centre. To the north and northwest of the centre the winds are usually light. To the south and southeast, the winds are moderate in force for a distance of sixty to ninety miles from the centre and then increase in intensity. Thus the most violent winds of these storms are in a great many cases found at a considerable distance from the centre, and only to the south and southeast. The main points of contrast between the cyclones of the transitional period and the cyclonic storms of the rains are given at length by Mr. Blanford.* The following, however, is a convenient summary by Mr. Pedler.†

"There are three or four important points in which the cyclonic storms of the rains differ widely from the cyclones of the transition periods. They are: —

"*First.* In the cyclonic storms of the rains the main strength of the wind is confined to the southern and southeastern quadrant, and in some cases winds are feeble in force for considerable distances round the centre of the storm.

"*Second.* The absence of the central calm area in cyclonic storms of the rains, which exists in the larger cyclonic storms of the transition periods in the Bay of Bengal, which, for convenience, are called below, true cyclones.

"*Third.* That with almost equal wind force, the differences of pressure from the outer area to the centre of the storm, in the case of the cyclonic storms of the rains, are very much smaller than in the case of the true cyclones.

"*Fourth.* The lowest pressure in the cyclones of the transition periods practically coincides with the centre of the storm and of the wind circulation, while in many storms of the rains the lowest pressure is many miles south of the centre of the wind circulation. It will be shown later on that, in the storm

* Climates and Weather of India, 84.

† Cyc. Mem., Pt. II, 1890, 124.

now being discussed, the lowest pressure was at one time, at least, fifty miles south of the centre of circulations of wind. . . .

"*Fifth.* The work which has been done during the past few years on the subject of cyclones and cyclonic storms has apparently proved that some storms are confined practically entirely to the lower layers of the atmosphere, and are thus entirely broken up and disintegrated when in their course they come in contact with even low ranges of hills. Other storms apparently extend to great heights in the atmosphere, and such storms pass over low ranges of hills in their paths almost unchanged in intensity, and the direction of their movement is quite unaffected by such obstacles. The storms which belong to the first class are usually the violent cyclones of the transition periods, while the storms of the second class are those usually of the rainy season."

We come now to the consideration of the cause of these storms of the rains; the question being whether to include them in storms of convectional or of dynamic origin. Upon this subject Mr. Blanford has expressed an opinion, as follows*:—

"It has been suggested to me by Prof. Hann that even if the severe cyclones of the transitional periods of the monsoons arise in the way I have above indicated, the milder but more lasting 'cyclonic storms' of July and August, which are generated in the extreme north of the bay, and which often traverse a great part of the whole of Central and Northwestern India before they break up, may nevertheless be formed in the same manner as those of the temperate zone. But this seems to me extremely improbable. With the single exception of the place of their origin, the circumstances of their formation are essentially identical with those of the former class. Moreover, the track which they almost invariably follow seems to be determined by the monsoon currents, being along the trough of low pressure which lies between the easterly and westerly branches of the monsoon of Northern India."

Considering Mr. Blanford's thorough knowledge of the subject gained from first-hand observation on the ground, and expanded by a broad acquaintance with physical and meteorological theories, it seems highly probable that such a decision upon the matter is correct. It is therefore with some diffidence that the writer, studying the subject from the other side of the globe, ventures to indicate briefly some of the reasons which, so far as accessible literature on the subject goes, lead him to the conclu-

* The Genesis of Tropical Cyclones, Nature, xliii, Nov. 27, 1890, 82.

sion that these storms are of the dynamical kind. If such a presentation serves only to draw forth fuller and more conclusive reasons why those who are familiar with the subject regard the storms as convectional, it will have sufficient excuse.

First of all there is a curious fact connected with the origin of these storms, which, while by no means amounting to a demonstration, yet seems to furnish a presumption in favor of the dynamical theory.

According to the convectional theory, an excess of temperature or an unequal distribution of aqueous vapor, produces the first slight deficiency of pressure; moist air is brought in and the latent heat of the condensation of its vapor into rain leads to a still further diminution of the pressure.

It does not seem, however, that this process takes place in the formation of a storm of the summer rains. In a region of high barometer the "pressure apparently first gives way to a marked extent."* The rainy monsoon current then rushes in, but with the continuation of the rainfall the barometer generally rises, until the inrush is shut off. After a few days the phenomena are repeated.† Thus the pressure, instead of being lowered by the condensation of vapor in the manner called for by theory, is apparently under the control of some force which successfully opposes the condensation.

A second point to be noted is the distribution of wind intensity about the centre. As already stated, the most violent winds of the cyclonic storms of the summer rains are frequently found at a distance of sixty to ninety miles from the centre; while nearer the centre the winds are quite light. The difficulty of reconciling this condition with the convectional theory cannot be better illustrated than by quoting an attempt to do so by Mr. Pedler.‡ It should be stated, however, that the article was written while the convectional theory was almost universally held.

"A little reflection will show what peculiar and interesting features these [conditions of wind distribution] represent. The general conditions of the cyclonic storms are now well understood, and it is proved beyond any reasonable doubt, that in

* Eliot: S. W. Monsoon Storms, 1877–1881. Ind. Met. Mem., II, Pt. 4, 426.
† Eliot: Ind. Met. Mem., II, Pt. 1, 1.
‡ Cyc. Mem., Pt. II, 1890, 122.

such a storm the winds blow in a spiral manner towards the centre, the winds being *drawn* towards a centre of low pressure which acts as a kind of axis round which the winds circulate. The whole of the evidence available shows that the energy which causes the winds to blow round and towards the centre exists in the aqueous vapor contained in the air, and this energy is liberated by its condensation into the torrential rainfall occurring within the storm area, and that therefore the winds are not caused by the action of any unusual force or cause acting *outside* the storm itself. It will therefore clearly follow that the further away from the inner area of the storm a particle of air is situated, the more feeble will be the force acting upon it and drawing it towards the centre of the storm; and the nearer to the centre, roughly speaking, the greater will be the force acting upon the particle. . . . It can also be understood how, when the winds from the outer and very large circumference of a storm area, acted on by forces which exist within the storm area itself, and which may be supposed to be greatest near the storm centre, pour in spirally and continually towards smaller circles near the centre of the storm, that the wind force must increase very materially, and that the winds will increase in intensity from the outer circumference of the storm towards the centre, the winds reaching their maximum force immediately outside the central calm area. That this is the case has been proved in abundant instances in the history of the cyclones of the transition periods, *i. e.*, when the storms form during a comparatively still condition of the atmosphere and when barometric pressures round the Bay are almost uniform. But in the case of the cyclonic storms of the rains, the history of which has been worked out in numerous cases by Mr. Eliot, Meteorological Reporter to the Government of India, there is usually no calm centre observed at all in the passage of the storm over the land. In very many cases also, though not in all, it is found that the winds are more or less feeble near the centre and for some considerable distance away from it. That is, over the area which in ordinary cyclones is the position of the fiercest winds, in the storms of the rains winds are sometimes of feeble character, while the strong winds are many miles away to the south or southeast of the centre, where in the intense cyclones of the transition periods winds are lighter than towards the centre.

"Is it to be admitted that there is in the cyclonic storms of the rains a reversal of the ordinary law of cyclonic action, or, that winds may be very strong in the outer area and feeble in the inner area, and that the very strong or hurricane winds to the south of the storm, as they gradually approach in a spiral direction towards the centre, lose their velocity so rapidly that there

is no trace of their existence to be found nearer than perhaps sixty or eighty miles from the centre, and that no strong winds are to be found to the east, north, and west of the centre of such a storm? This can scarcely be admitted, for the cyclonic storms of the rains and the cyclones of the transition period are formed by similar causes and only differ in details of severity, size, etc."

This conclusion appears somewhat illogical, inasmuch as the writer is apparently driven to the disparagement of the facts clearly defined by himself and Mr. Eliot, because they are not explained by the convectional theory. The only point, however, with which we have to deal is that the facts are not in accord with the theory. This seems to be proved sufficiently for acceptance.

The persistency of these cyclonic storms in passing over hills may also be used as an argument against their convectional origin. And it should be remembered that by "hills," those who dwell in the shadow of the Himalayas designate elevations as high as two thousand or three thousand feet, the average height of our Appalachian mountains. Such ranges would provoke considerable rainfall from low, moist currents passing over them, leaving little supply of energy for the storm centre; and yet the cyclonic storms frequently encounter and surmount hills of this height.

Last of all, the height of these storms is indicative of their dynamical origin. We usually consider convectional action to be a phenomenon of the lower atmosphere. Indeed, it is not difficult to calculate, in meteorological terms, how high a convectional storm may be. When the bottom air is heated to the point of overturning, the height to which it will ascend depends largely upon the amount of aqueous vapor it contains. If this amount is great, there will be a large amount of latent heat to be liberated, the cooling of the ascending air will be reduced to a minimum, and the air will ascend to a considerable height.

Now, in the formation of the transitional cyclones, as described by Mr. Blanford,* we have a great mass of air almost at the point of saturation. During the storm the rain falls in torrents. There is liberated the greatest possible amount of latent heat, and it would seem that the height to which this air would go

*Genesis of Trop. Cyclones, Nature, xliii, Nov. 27, 1890, 81.

must represent almost the maximum for convectional storms, yet this height is far less than that of the cyclonic storms.

In four points then, — in their formation, the distribution o wind intensity, their persistence, and their height, — the storms of the summer rains appear to belong rather with the dynamical storms of the winter rains than with the convectional cyclones of the transition periods. As already stated, however, these reasons are not presented as conclusive, but are rather designed to open the question for a fuller discussion than it has as yet received.

HARVARD COLLEGE, CAMBRIDGE, MASS., JUNE, 1892.

THE FIRST AERIAL VOYAGE ACROSS THE ENGLISH CHANNEL.

R. DE C. WARD.

IN the June number of this JOURNAL the writer gave a brief account of the first scientific balloon voyage, which was made by Dr. John Jeffries, of Boston, Mass., U. S. A., and M. Blanchard, a Frenchman, on Nov. 30, 1784, from London. In the present article it is proposed to give some account of the first aerial voyage across the English Channel, which was successfully carried out by Dr. Jeffries and M. Blanchard in the year following this.

Not content with his short ascent in 1784, Dr. Jeffries determined, in order to carry his scientific investigations further, to make a trip by balloon across the channel. He therefore made an agreement with Blanchard "to furnish him with all the materials and labour to fill the balloon, and to pay all the expenses of transporting them*" the amount thus expended amounting to over £800.

The two aeronauts left London on Dec. 17, 1784, but were

* "A Narrative of the Two Aerial Voyages of Doctor Jeffries with Mons. Blanchard; with Meteorological Observations and Remarks. The first voyage, on the thirtieth of November, 1784, from London into Kent. The second, on the seventh of January, 1785, from England into France. By Doctor Jeffries. Presented to the Royal Society, April 14, 1785; and read before them January, 1786. London: Printed for the author; and sold by J. Robson, New Bond Street, MDCCLXXXVI."

delayed at Dover in repairing and adjusting their apparatus, and by stormy weather, until Jan. 7, 1785, "when," Dr. Jeffries remarks, "after having encountered, almost from the hour of my arrival at Dover, a variety of discouragements and oppositions to the accomplishing my design, the weather being more favourable, we determined to proceed, by filling the balloon, and attempt our enterprize." The morning was fine and clear, with a wind northnorthwest or northwest by north, which the pilots at Dover said did not extend far from the shore. Although "somewhat embarrassed at this idea from professional men," Dr. Jeffries noticed that the light upper clouds were moving in the right direction, as was the smoke from the castle, so they determined to proceed with their preparations. After breakfasting with the Deputy Governor in the castle, and changing his clothes for the journey, Dr. Jeffries went to the balloon, which he found already half filled. A kite, a paper Mongolfier, and a small gas balloon were sent up, all of which took a favorable course over the channel, so that the hopes of the aeronauts began to brighten.

The balloon was filled a little before one o'clock, and then the wings and oars were attached. The ascent is thus described by Dr. Jeffries: "Exactly at one o'clock (having in the car with us three sacks of sand ballast, of ten pounds each; a large parcel of pamphlets, two cork jackets, a few extra clothes of M. Blanchard, a number of inflated bladders, with two small anchors or grapnels, with cord affixed, to assist our landing) we rose slowly and majestically from the cliff, which being at the time of our ascent from it almost covered with a beautiful assembly from the city, neighbouring towns and villages, with carriages, horses, etc., together with the extensive beach of Dover, crowded with a great concourse of people, with numbers of boats, etc., assembled near the shore, under the cliffs, afforded us, at our first arising from them, a most beautiful and picturesque view indeed." On this voyage Dr. Jeffries took a barometer and a mariner's compass, but "no other philosophical instrument," his chief object being, as he said, "the novelty and enterprize of being one of the first who passed across the sea from England into France, by the route of the Air." At the instant they rose from the cliff the barometer marked 29.70 in., and the wind was north-northwest, though nearly calm. By a quarter

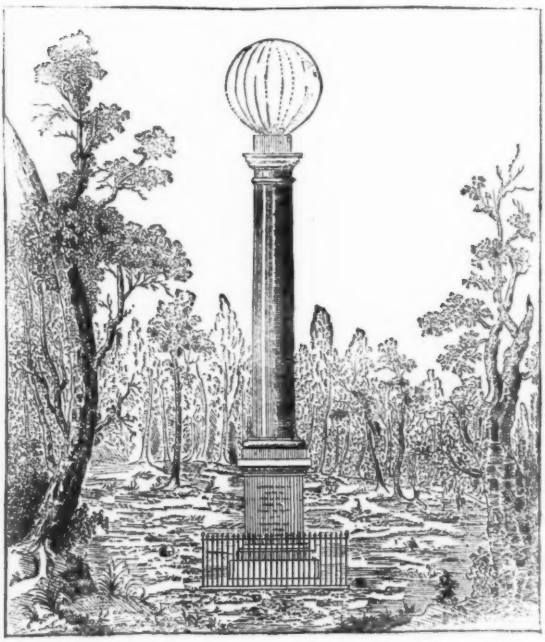
past one the barometer had fallen to 29.30 in. The view back towards Dover and forward towards France was noted as enchanting. At fifty minutes past one the balloon was found to be descending fast, so ballast was thrown out until the mercury began to fall again. At two o'clock, when the balloon was half way across, it became necessary to cast out all the remaining ballast, as well as a parcel of M. Blanchard's books. "Forty minutes after two . . . found ourselves descending, and very rapidly, — the part of the balloon next to us having collapsed very much, apparently for many feet from the lower pole. We cast out all the little things we could find, — apples, biscuit, etc., then one wing; still descending we cast away the other wing; but not rising, cut away the damask curtains around the car, with the gold tassels, etc., then stripped off all the silk lining, threw out our bottle of l'eau de vie. In its descent it cast out a stream like smoke, with a rushing noise, and when it struck the water, we heard and felt the shock very perceptibly in our car and balloon."*

Finding that the balloon was still descending they began to strip themselves, and put on their cork jackets, "prepared for the event." Suddenly Dr. Jeffries noticed that the mercury was falling again, and soon the balloon ascended to a greater height than it had reached during the voyage, the barometer marking 23.30 in., the cold at that height being keenly felt by the occupants of the car, who had thrown away most of their clothing. The wind soon increased in force, and the balloon began to descend again, taking a course towards a large forest, across which it moved with great velocity. The cork jackets were thrown out, as well as everything that still remained of weight in the car, and at exactly quarter past three Dr. Jeffries caught hold of the top of a tree in the forest and arrested the further progress of the balloon. At a few minutes before four the voyagers were able to alight in what they found was the Forest of Guines, not far from Ardres, and near the spot celebrated for the famous interview between Henry the Eighth of England and Francis the First of France.

The aeronauts were received with the greatest attention and civility on the part of the people in the neighborhood, and were

* Dr. Jeffries' Private Dairy.

entertained in state by the Mayor and the military and civil officers of Calais. They were escorted to the latter city in carriages, and although they arrived at the gates after midnight, they were admitted by special orders. On his return to England



Dr. Jeffries was made a Baron of the Cinque Ports, and Honorary Freeman of the town of Dover. He remained in that country four years, in practice in London, and then returned to his native city, where he practised medicine until 1819, when he died.

One of the closing paragraphs in Dr. Jeffries' narrative is as follows:—

"Although I recollect, in our former aerial voyage, to have noticed it at the time, yet as I had omitted it in my memorandum made during our progress, I cannot but mention here, that amidst all the magnificent and extensive scenes under and around me, nothing at the time more impressed me with its novelty than (if I may be allowed to use the expression) the awful *stillness or silence* with which we seemed to be enveloped, which produced a sensation that I am not able to describe; but which seemed at the time to be a certain kind of stillness (if I may so express it) that *could be felt*."

On February 17, 1785, the "*Corps municipal de la ville et Comte de Guines*" voted, under Royal consent, to erect a monument at public expense on the spot where the balloon landed. This monument is shown in the accompanying plate. In the course of years the memorial was shorn of the tablets which commemorated the voyage, but in 1885 it was restored on the occasion of the centennial anniversary of the great event. On May 23 to 25 of that year the inhabitants of the town of Guines celebrated the one hundredth anniversary of the ascent with a general holiday, games, festivals, a balloon ascension, and like festivities. It is interesting to note that two great-grandsons of Dr. Jeffries were present on this occasion as guests of the town. A public pilgrimage was made to the column in the forest, new tablets were affixed to it, and wreaths were placed on it. The town of Guines also struck a medal in honor of the event.

The writer is indebted to Dr. B. Joy Jeffries, of Boston, for the loan of the books from which the above abstract has been compiled, as well as for the loan of the plate for the accompanying figure. The barometer and mariner's compass mentioned above are now in the possession of Dr. Jeffries.

ON THE PRODUCTION OF RAIN.*

PROF. CLEVELAND ABBE.

MAN may not hope to assist nature in the formation or prevention of rain until he better understands the details of nature's own methods. The ideas most widely accepted, at present, as to the natural process of the formation of rain, go only so far as to say that the moisture present in the atmosphere is extracted from the air by three different steps; namely, first, the saturation with aqueous vapor that is produced by cooling the air; second, the condensation of the vapor into small visible particles of cloud and fog; third, the agglomeration of these droplets of water into drops large enough to be precipitated as falling rain, hail, or snow.

I. The ordinary natural methods of accomplishing the cooling required in the first of these steps are: (a), the mixing of cold and warm air, by which, however, only a very slight amount of precipitation can be formed; (b), the radiation of heat to the colder earth and air and space, by which, at first, thin layers of fog or stratus clouds are formed, which slowly thicken with time; (c), the rise and expansion of large masses of air: the mechanical work done by the expansion, simultaneously, of the whole mass may cool it to any extent whatever. This last is the important process on which all our rain depends.

II. The second step, namely, condensation, is a molecular process that has been likened to the crystallization of solid salts from liquid solutions, although there is too little known about either process to warrant the belief that they are really similar. Aitken and others maintain that the condensation of vapor, like the crystallization of salts, demands some nucleus as a starting-point, and that every minute droplet of fog or cloud must have a particle of atmospheric dust as its initiative.

* Extract from *Agricultural Science*, Vol. VI., July, 1892.

III. The third step in the above process of rain formation is the agglomeration of fog or cloud-particles into larger drops. About this there is very little known from actual observation, and the hypotheses are quite various. The hypothesis that among these particles some are larger than others, and, by their more rapid descent, overtake the smaller ones and thus grow larger as they descend, seems at first quite natural and is sufficient to explain the fact that the quantity of rainfall is an exceedingly small percentage of the water that is visible as a cloud, and, of course, a still smaller percentage of the water that is present as vapor in the air. On the other hand, microscopic observations of the sizes of the particles of fog do not show a variation in the diameters sufficient to allow of one particle falling much faster than its neighbor; therefore, as the air always has a motion sufficient to carry these minute particles along with it, it would seem that if they are to come in contact and form larger particles, it must be through a process of jostling together rather than through a process of falling by gravity.

But the contact of two particles, whether by gravitational fall or by the jostling of wind-currents, will not necessarily cause their union; it is essential that the surface tensions of the two particles be properly adjusted to each other, and this latter point seems to demand further study. It is true that by proper electrification we are able, in the laboratory, to alter the surface tensions and to cause small fog particles to coalesce; but it is not so evident that this is the ordinary process of nature. We are also able in a laboratory to alter surface tensions by surrounding the particles with a different gaseous or vaporous atmosphere, or even by changing the temperature of the particles; but neither of these processes is likely to be the process that takes place in the clouds. Rayleigh's experiments on jets of soapy water make it plausible that a slight impurity in the rain water, such as ammonia or nitric acid, may make it possible for certain cloud particles to agglomerate, while the neighboring droplets of pure water would not do so. Again, a thin film of foreign substance, such as oil on the water, lowers the surface tension and allows two such oily drops to combine into one, when the film is broken at any point, by pressing the drops close together. But none of these laboratory experiments seem applicable to the formation of the natural cloud and rain.

As to these various hypotheses that have been suggested concerning the method by which the agglomeration of droplets into large drops is actually effected by nature in her regular process of making rain, I must remark that it is not yet clear to me that any one has demonstrated that small drops actually do agglom erate into larger ones to any considerable extent. I think it quite possible that the union of small cloud particles into larger ones is only effective in drizzling fogs or clouds whose upper surfaces cool by radiation ; but is, after all, not an important feature in the natural production of generous rains and summer thunder-showers. It is a reasonable "working hypothesis" that the particles, which were originally too small to fall from the clouds with any rapidity, actually remain there entangled in the currents of air that characterize clouds, and that they are subsequently evaporated ; while, on the other hand, only those fall as rain which, originally, had a size vastly larger than the average size of the smaller particles that constitute the major portion of a cloud. There may be some reason why the condensation of the superabundant molecules of a saturated vapor should form, not merely cloud particles whose diameter is ordinarily less than one one-hundredth of an inch, but also, here and there, large drops which fall to the ground as rain with very much the same size as when originally formed a few moments before in the clouds. The sudden pour of heavy rain from a limited region within a thunder cloud, cannot be due to a general slow progressive agglomeration of droplets into drops.

On this point I submit the following modification of ideas suggested by reading von Bezold's fourth paper on the "Thermodynamics of the Atmosphere," Berlin, 1892; it suggests a new point of view, and one that demands further experimental elucidation.

Bezold suggests that the heavy rains generally known as cloudbursts are immediately preceded by, and may owe their origin to, a supersaturated state of the atmosphere, by reason of which a greater quantity of vapor is contained in the air than would, under other conditions, be possible at a given temperature. Following out this hypothesis, I conclude that, whatever molecular condition it be that permits the existence of a supersaturated atmosphere, it is evident that the removal, or annulment, of that condition must give rise to an immediate and heavy con-

densation. This principle may be extended to all ordinary rains as well as to the violent thunder-showers and cloud-bursts.

The supersaturated condition must be considered as a case of adiabatic expansion * accompanied by a delay in the occurrence of the appropriate condensation; so far as we at present know, this condition can occur only in those cases where all foreign substances, or dust particles, are absent, which might serve as nuclei for the formation of fog particles. A slowly rising and cooling atmosphere first condenses its moisture on the dust nuclei and forms minute droplets; these grow very slowly, by diffusion, up to a definite size proper to the prevailing temperature and vapor tension, but the intermediate air, in which these droplets are floating, keeps on cooling as a dust-free, supersaturated vapor. If the sun shines on these droplets its heat powerfully contributes to evaporate them and further to saturate the surrounding air.

In general, therefore, the ascending portions of every cloud contain supersaturated, dust-free vapor, separating the isolated droplets. When by further expansion and cooling the supersaturation has proceeded to such an extent that further condensation must occur, this latter molecular change permeates the supersaturated space with a rapidity comparable to that with which any other chemical change takes place, just as when the explosive union of chlorine and hydrogen, or of oxygen and hydrogen, starting at any one point, almost instantaneously permeates a mass of those mixed gases, or, as when combustion runs along a train of gunpowder. The vapor molecules from the supersaturated spaces are quickly brought together by their molecular attractions into heavy drops of warm water, which are often distinct from the intermediate cooler droplets and descend rapidly from the clouds, while the latent heat of condensation is communicated to the adjoining air and is left behind in the cloud. Thus simultaneously with the formation and fall of the big drops there is a sudden expansion of that portion of the clouds from which they came; Bezold thinks that such expansion may possibly be felt at the earth's surface as a sudden rise in the barometer, while it is also visible to the eye as a sudden expansion of the cloud into the so-called "thunder-head." I

* *I. e.* without adding or subtracting heat.

myself doubt whether there would be any appreciable barometric result, yet I consider that the sudden expansion and ascent of the white cloud and its subsequent rapid dissipation into the surrounding air, together with the simultaneous lightning, thunder, rain, hail, and ascending whirl and wind, all conspire to make it very plausible that there really existed a supersaturated condition at the moment immediately preceding.

If the temperature of the dew-point of the ascending air, or the temperature of supersaturation is below freezing, the condensation of the vapor may at once form, not drops, but large snow-flakes, such as will fall rapidly to the ground, or the small hail that is ordinarily called sleet.

Correlated phenomena occur when a cloud consists of small particles of water cooled below the freezing point, as is known to be frequently the case. When for any reason these particles are suddenly converted into ice, as will happen when they are cooled low enough or when they jostle against each other, their temperatures at once rise to the freezing point, a large quantity of heat is set free, the cloud expands and rises and the droplets of water are converted into spiculae of ice, or small snow-flakes; large flakes and hailstones are not to be explained in this manner.

There is some plausibility in the hypothesis that the critical electrical condition, which results in lightning, is directly due to the disruption of the condition of extreme supersaturation and the sudden formation of large drops of water, or the disruption of the condition of water cooled below the freezing point and the consequent sudden formation of ice or snow; but this remains to be investigated.

Therefore, according to this latest view of the subject, the problem of the artificial formation of rain will be partially solved, and sufficiently so for practical purposes, if some method is invented by which to bring about a sudden formation of a small percentage of large drops out of the moist air that exists between the small particles of every cloud.

At present, our attention and experiments, should be directed toward understanding and completing the natural but obscure process involved in the formation of rain within the cloud, and not toward the forcing of any unnatural process.

CURRENT NOTES.

The British Association at Edinburgh. — The following are abstracts of the principal papers relating to meteorological matters, in addition to those noted in the October number of this JOURNAL, which were presented to the British Association for the Advancement of Science at its Edinburgh meeting, Aug. 3 to 10, 1892, and of the discussions which followed the reading of those papers: —

Professor Arthur Schuster, F. R. S., president of the Mathematical and Physical Section, during his address, said: That some of the results recently brought to light by investigations on the discharge of electricity had interesting cosmical applications. Thus it was found that such a discharge through any part of a vessel containing a gas converted the whole gas into a conductor. The dissociation, which we imagined to take place in a liquid before electrolytic conduction took place, must be artificially produced in a gas by the discharge itself. There were other ways in which a gas could be put into that sensitive state in which we might treat it as a conductor, and we had every reason to suppose that the upper regions of our atmosphere were in that state. The principal part of the daily variation of the magnetic needle was due to causes lying outside the surface of the earth, and was, in all probability, only an electro-magnetic effect due to that bodily motion in our atmosphere which showed itself in the diurnal changes of the barometer. A favorite idea of the late Professor Balfour Stewart would thus probably be confirmed. The difference in the diurnal range between times of maximum and times of minimum sun-spots was accounted for by the fact that the atmosphere was a better conductor at times of maximum sun-spots. The mention of sun-spots raised a point not altogether new to the section. Careful observations of celestial phenomena might suggest the solution of many mysteries which were now puzzling us. Consider, for instance, how long it would have taken to prove the universal property of gravitational attraction if the record of planetary motion had not come to the philosopher's help. And surely the most casual observation of cosmical effects taught us how much we had yet to learn. The statement of a problem occasionally helped to clear it up; and he might be allowed, therefore, to put before them some questions, the solution of which seemed not beyond the reach of our powers. 1. Was every large rotating mass a magnet? If it was, the sun must be a powerful magnet. The comets' tails, which eclipse observations show stretching out from our sun in all directions, probably consisted of electric discharges. 2. Was there sufficient matter in interplanetary space to make it a conductor of electricity? He believed the evidence to be in favor of that view. 3. What was a sun-spot? It was

generally, he believed, assumed that it was analogous to one of our cyclones. The general appearance of a sun-spot did not show any marked cyclonic motion, though what we saw was really determined by the distribution of temperature, and not by the lines of flow. But a number of cyclones clustering together like the sun-spots in a group should move round each other in a definite way, and it seemed to him that the close study of the relative positions of a group of spots should give decisive evidence for or against the cyclone theory. 4. If the spot was not due to cyclonic motion, was it not possible that electric discharges setting out from the sun, and accelerating artificially evaporation at the sun's surface, might cool those parts from which the discharge started, and thus produce a sun-spot? 5. Might not the periodicity of sun-spots, and the connection between two such dissimilar phenomena as spots on the sun and magnetic disturbances on the earth, be due to a periodically recurring increase in the electric conductivity of the parts of space surrounding the sun? Such an increase of conductivity might be produced by meteoric matter circulating round the sun. 6. What caused the anomalous law of rotation of the solar photosphere? It had long been known that groups of spots at the solar equator performed their revolution in a shorter time than those in a higher latitude; but spots were disturbances which might have their own proper motions. The exceptional behavior of the solar surface seems to deserve very careful attention from solar physicists. Its explanation will probably carry with it that of many other phenomena.

A joint meeting of Section B (Chemistry) and Section E (Geography) was held, in order to discuss some questions of chemical oceanography. Mr. J. Y. Buchanan, F. R. S., Cambridge, communicated some new observations on sea water density in the deep-sea off the east coast of Brazil. These indicate that the depressions in this region are filled with water which has resulted from the melting of the sea-water ice of the Antarctic regions. Dr. Buchanan pointed out the extreme importance of the results from the standpoint of meteorology. The second paper was by Prof. Otto Pettersson, Stockholm, and dealt with the hydrography of the Baltic Sea and its approaches. By means of large maps, Professor Pettersson described the distribution of density over the area, and deduced the general circulation of the water. In summer a strong current of brackish water flows out from the Skagerak, and sweeps northward round the coast of Norway; in winter, when the rivers are frozen, this overflow ceases, and a comparatively warm and salt current sweeps from the North Atlantic through the Skagerak, and simultaneously the winter herring fishing begins. When the cold fresh current resumes its course in spring, the herring fishing along the shores ceases. Dr. M. N. Andresoff, St. Petersburg, read a paper, in German, on the recent Russian explorations in the Black Sea. The most remarkable discovery made was that, below the depth of two hundred fathoms, the water contained so large a proportion of sulphuretted hydrogen that no living creatures were found in greater depths. Dr. John Gibson read a paper on the analysis of sea water, in which he expounded certain chemical differences, which enabled him to distinguish the origin of water coming from

different regions. Mr. H. N. Dickson corroborated Dr. Gibson's results from his own observations on the English Channel.

Dr. A. Buchan gave to the Physical Section a preliminary account of ocean circulation based on the "Challenger's" observations. He explained that he had been engaged upon the work for a long time, and the inquiry had so far advanced that the more prominent results could be stated. These he submitted in an oral report. The "Challenger" observations had been supplemented by those of Mohn, Agassiz, J. Y. Buchanan, Belknapp, and Captain Wharton, the hydrographer. Dr. Buchan emphasized the importance of the accurate study of the surface winds of the globe, as bearing on the subject of ocean temperature. He referred particularly to the directions of the winds of the Atlantic, the result of the observations going to show that from the ocean currents thereby generated, the temperature on the west side of the Atlantic, at depths from one hundred to five hundred fathoms was nearly ten degrees warmer than at the same depths on the east side. At five hundred fathoms the temperatures on both sides of the Atlantic were nearly equal, but at lower depths they were reversed, the warmer water now being on the east side, and the colder on the west side. The west side at these depths was more under the influence of the Arctic currents along the American coast, and the east side was more under the influence of the under currents from the Mediterranean and the equatorial regions of the Atlantic. This high temperature was carried northward even beyond the Wyvil Thomson ridge, which lay between Shetland and Iceland. Indeed, at depths of seven hundred fathoms the temperature immediately to the south of this ridge was five or six degrees warmer than over the Pacific, Indian, and South Atlantic oceans at the same depth. The temperature of the Mediterranean at two hundred fathoms was about fifty-six degrees. From this point down to the bottom, which extended in some places to one thousand five hundred fathoms, there was no change. Similarly, in the Gulf of Mexico below seven hundred fathoms there was no change. On the other hand, in the North Atlantic, north of the Wyvil Thomson ridge, at all depths below seven hundred fathoms, there was a uniform temperature of about twenty-nine and one half degrees, which was two or three degrees higher than the freezing point of water of the sea there. The bearing of the under current of the Mediterranean of warm salt water on the currents of the Atlantic seemed to him to fully explain why history gave no instance of an iceberg appearing off the west of Europe, this warm southerly undercurrent extending beyond the North Cape of Norway.

Mr. H. N. Dickson, Edinburgh, described the physical conditions of the water in the English Channel. The observations had been taken west of a line covering the Isle of Wight and Guernsey. It had been found that the constitution of the water agreed in the main with the "Challenger" samples, and was found to be Atlantic water entirely. The tidal currents in the Channel were sufficiently strong to keep the water in a thorough state of mixture from the surface to the bottom, except off Start Bay, where the tides formed a vortex in which the water was colder in summer and warmer in winter than in the rest of the area. Start Bay was one of the best fishing

grounds in the channel, and the same superiority as a fishing ground on parts of the west coast of Scotland where similar conditions prevailed had been noted.

The Prince of Monaco read to the Geographical Section a paper on the currents of the North Atlantic Ocean. He referred to his prolonged experiments in recent years with floats in the Atlantic. These were thrown into the water, and there was an arrangement for their identification on being re-discovered. The current in the Atlantic, which he had demonstrated and examined, was a circular or vortex one, the centre of the circle, which was several hundred miles in diameter, being a short distance to the south-west of the Azores. The floats which entered this region remained in it for months and years. Some of his floats arrived on the coast of France after an interval of over four years. The only outlet found by the floats from this vortex had been to the northeast towards the coasts of France, Britain, and Norway. The results of his observations enabled him to offer figures as to the speed of the currents which were approximately exact. He had prepared a table showing the geographical and annual distribution of all the floats recovered, and the mean velocity of the floats. In all cases where there was any doubt, he had not employed the results in drawing his conclusions. The mean velocity of the current in various directions about the central region round the Azores varied; but the mean velocity which the combined results gave for the North Atlantic was 4.48 miles in the twenty-four hours. It was apparent that circulation was more active on the western half circuit of the vortex than on the eastern one, and this was explained by the combined action of various causes, as the trade winds, the equatorial currents, the gulf stream, and the powerful evaporation of the tropics which stimulated the circulation of the water.

The Prince of Monaco proceeded with another paper, in which he showed the importance of having observatories on various islands in the Atlantic. He said that the western shores of Europe, especially those of Great Britain and Ireland, were exposed to continual devastations by storms from the westward. The only source from which we could obtain warning is the continent of North America, and the warnings thence received were of the greatest value. But the width of the North Atlantic was so great that American storms lost themselves, and others were generated, of which Europeans could at present receive no warning. The North Atlantic was fairly rich in islands, and in nearly every group there was at least one island of remarkable altitude, where the meteorological conditions were likely to be quite distinct from those obtaining at the sea-level. The results already achieved at the high-level observatory on Ben Nevis justified the confident expectations that equally, and perhaps more valuable, results would be obtained by the establishment of a pair of high and low level observatories in one of the Atlantic groups of islands, preferably the Azores. These groups of islands either were already, or would be shortly, connected by cable with Europe; and he strongly advocated that they commence now to establish meteorological observing stations at carefully selected sea-level stations having cable connection with Europe, and at least one high-level

observatory at the top of one of the remarkable peaks named. Further, as it might be advantageous to have these observations collected at an observatory where much attention is given to oceanographical work, he offered to undertake at Monaco the collection and discussion of the observations received daily, and the distribution of the results. He showed that by these island observatories results might be obtained free from all the disturbing influences associated with masses of land. The situations of the proposed observatories were Bermuda, the Azores, Madeira, the Canaries, Teneriffe, and one of the Cape Verde Islands. Some would be at a high level, and some near the surface of the sea. If the plan he had conceived were carried out, he was convinced it would have a very great effect in furthering the advances of the sister sciences. He was willing that the results of the observations should be collected at Monaco where the Government would undertake the discussion, distribution, and publication. There was already a meteorological observatory there, under very scientific direction. He proposed that next winter delegates of the interested nations should meet at Monaco to discuss those important questions.

Sir Archibald Geikie, president of the Association, spoke of the immense practical importance of the Prince's proposals. If such a system of observatories were adopted, there seemed every probability that warnings of storms would be given in ample time.

A discussion on the two foregoing papers then took place.

Dr. Buchan said the results of the investigations of the Prince of Monaco were extremely interesting. One cause of the greater velocity in one part of the Atlantic vortex was that the wind blew in the same direction for a long time, and the winds were often stronger than at the other side of the vortex. But more than that, in working out the subject of oceanic circulation, it was found that on the west side of the Atlantic the surface-water was accompanied, down to a depth of nine hundred fathoms, by a much warmer temperature than on the east side. There was a difference of ten degrees, which he thought was due mainly to the upper current of warm water coming from the Gulf of Mexico. Coming to the prince's second paper, he said that, when the temperature of the surface-water of the Atlantic, in late autumn, was several degrees above the average of the season, the result was a higher temperature and a more active evaporation from the surface of the sea.

This had, in a number of cases, been followed by the storms of the subsequent winter and spring taking a more southerly course through Europe than usual, thereby placing the inhabitants of the British Isles on the north side of the centres of those cyclones, and thus exposing them to a hard winter and spring, with prevalent northerly and easterly winds and snow storms. In these circumstances, what they wanted was a first-class observatory at Bermuda. He also strongly urged the laying of a cable to the Azores, so that information as to the weather might be obtained. He had examined many weather reports, and it was plain that a very considerable number of storms were felt at the Azores, on their way to Europe, particularly those storms that entered Europe at lower latitudes than usual. That was to say, a low-level observatory on the Azores would supply more definite

information as to east winds and severe winter weather when we were on the north side of a cyclone. As to a high-level station, one of the most valuable results already obtained was that there were two kinds of cyclones that were well marked out by the observations at the top of Ben Nevis. One was a storm or cyclone where the winds at the top of the mountain were exactly blowing in the same direction as the winds at the sea-level all round. That was to say, the storm prevailing at the sea-level prevailed at least higher in the atmosphere than the top of Ben Nevis. It was part and parcel of the same great atmospheric whirl. Curiously enough, in these conditions there was a different type of air as regarded dust. But there was another and a totally distinct kind of cyclone, in which the winds at the top of Ben Nevis blew exactly in the teeth of those at sea-level, and the quality of the air was all but dustless. Sometimes the air was so saturated at the top of the mountain that a man would be drenched to the skin in two minutes. The man became a nucleus round which the vapour condensed. If they had a similar mountain observatory in the Azores, the combination of its observations with those of Ben Nevis would give a certainty as to what was happening in the way of weather to the westward. The combination would give the whole of the meteorological bureaus of Europe a table of observations to go upon that would enable them to perfect their forecasts. He suggested that they should begin with a low-level station at Bermuda, and afterwards at the Azores. Once they got the money for a high-level station at the Azores, its utility would be so manifest that there would be less difficulty in getting the funds to provide additional high-level stations.

Mr. A. L. Rotch, Boston, said the United States was now receiving daily reports from Bermuda, as there was a cable to Halifax, but he thought the station was not first-class. The establishment of observatories with cable connection, such as the Prince of Monaco had proposed, would be of the greatest service to forecasts, not only in the United States, but more especially in the various countries of Europe. A less extensive network of stations had been discussed at the International Meteorological Conference at Munich, last year, and although its desirability was generally admitted, yet the attendant expense was regarded as rendering its execution impracticable. He sincerely hoped that the Prince's scheme might be carried out.

"Can spiders prognosticate weather changes?" was the subject of a paper to the zoölogists of the Biological Section, contributed by Dr. McCook, Philadelphia. After apologizing for introducing such a subject into the serious work of the Section, he said he thought it worth while sometimes to make a thorough examination of popular opinions, especially when these became grafted upon popular scientific literature. It was a general opinion, very widely distributed, that spiders had the power of prognosticating the weather. The belief was current as early as the days of Pliny, who not only believed that these creatures were sensitive to changes of the weather, but declared that somehow they were able to tell when a river was about to rise, for then they built their webs higher in order to escape the disturbance from the stream. The late Rev. J. D. Wood stated in his writings that the British people, the common people at least, entertained the idea

that spiders were quite good barometers ; that they knew when a storm was impending and ceased to build their webs; while, on the other hand, when they were found mending or making their snares, it was pretty manifest that there would be fine weather. This was substantially the opinion as it had been found in America. He had often heard it expressed by the farmers, particularly throughout the Middle States of the United States, that the spiders were very good weather prophets, and preferable to the weather indications proceeding from Washington. Their idea was that when the webs appeared very numerously in the morning there would be fine weather, and, on the contrary, when the webs were not seen abundantly the weather was apt to be foul. After dealing at greater length with the popular opinion, he went on to state his method of inquiry into its truth. In the first place, he made many independent observations, particularly in various parts of the United States, and one or two in Europe, and carefully noted these observations as to the industry of the spider in relation to the condition of the weather before and after. He then introduced into his premises in Philadelphia a number of spiders drawn from the neighborhood, and some he had had bred from cocoons sent from the Pacific coast. He had quite an assemblage of spiders with which he carried on observations for some six years. From the records of these observations he made several quotations, from which it appeared that spiders took no account of the weather, but spun their snares in bad weather as in good. He had, therefore, come to the conclusion that so far at least as web weavers were concerned — for these observations were confined entirely to web weavers — there appeared to be no ground for the opinion that spiders were able to prognosticate the weather. The popular opinion might, therefore, be considered to be exploded. In reply to a question, Dr. McCook emphasized the point that these observations had been confined to web weavers. There was another group of spiders which made a broad-sheeted web, and upon it farmers in America very largely depended for their observations on the weather. The morning dew made those webs in the fields manifest where they had not been seen before, and as in America, when the dew was heavy in the morning, they were almost certain to have a fair day, if a man saw the spider webs in the morning, he concluded the day would be fine. This, however, did not show that the spider could prognosticate, for this was a group which made a web which would continue practically intact for a week, two weeks, or even a month. Prof. Rutherford said he was not at all convinced that Dr. McCook's conclusion was perfectly correct. He had not shown that spiders could not prognosticate. Like the members of the Association, the spiders might go on doing their work from day to day, and still, like them, be quite able to tell what the state of the weather might be a short time afterwards.

Atmospheric Depressions and their Analogy with the Movements of Sun-Spots. — The solar photosphere, although so different in chemical composition from our own atmosphere, yet affords us many points of resemblance with regard to its general circulation. One special analogy, and that a com-

p arison of the motions of storms here with those of spots on the solar surface, is treated of in the July number of *L'Astronomie* by M. Camille Flammarion. In this article he has brought together sufficient observations to trace out the paths of many of the most violent storms that have from time to time vis ited Europe generally. The first storms which he gives are those which occ ur in the Atlantic; their general direction of motion seems to be from south west to northeast, pursuing generally the path of the Gulf Stream. Th eir centres, when traced on a map, seem to just graze the shores of the British Isles, France very rarely being reached by them. From observations made on land, and more especially from those at Paris, M. Flammarion remarks that certain curves with regard to these storms seem to offer many analogies to solar spots; this is so not only for the regular displacements, but even for those which at first sight seem to be totally void of all regularity. The diagrams which he gives, showing both the paths of the storms and those of the sun-spots, afford most interesting comparisons, and seem to confirm the view suggested by M. Faye that the constitution of spots resembles somewhat that of the cyclones with which we are familiar. — *Nature.*

OBERBECK'S REVIEW OF MARCHI.

The following translation was already stereotyped when we were apprised that the publication of Marchi's full memoir had been delayed in order to correct a numerical error that had been detected by the author. It would seem, however, that neither this error nor the weakness of the deductive analysis, as criticised by Oberbeck, can diminish the importance of the empirical laws announced by Marchi. The students of meteorology in America will, undoubtedly, be interested in applying these laws to our own storms, wherefore we will not delay to publish this preliminary announcement of Marchi's results. C. A.

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DE MARCHI, LUIGI: Sulla teoria dei cicloni. Nota preventiva. Rendiconti del R. Instituto Lombardo: Series 2, Vol. XXV, Fascicle V; 8vo, 15 pages. Milan, 1892.

OBERBECK. Review of the above as published by Prof. A. Oberbeck, of Greifswald, in the Meteorologische Zeitschrift. Vol. IX (Z. O. G. M. XXVII) page [49]. [A full translation of this review is as follows: C. A.]

The complete memoir (a short extract from which is given in the above-quoted Rendiconti) will be published in 1893, and forms a continuation of the "Researches on the Mathematical Theory of the Winds," by the same author, which were published in the years 1882–1885. (See the summary by A. Sprung, pages 151–157 of his Treatise on Meteorology, Hamburg, 1885). A portion of the assumptions adopted in the earlier works of Marchi are here also retained, especially the neglect of the vertical component of motion in comparison with the horizontal movements. The starting point of the author's studies is expressed in an equation whose derivation is not explicitly given by him. But we easily deduce this formula from the fundamental equations of hydro-dynamics, [see, for instance, Sprung, "Meteorology," pages 132 and 153, or "The Mechanics of the Atmosphere," soon to be published by the Smithsonian Institution, pages 36 and 154]

$$\text{(A)} \qquad \begin{aligned} \frac{du}{dt} &= -\lambda v - ku - \frac{1}{\rho}\cdot\frac{\partial P}{\partial x} \\ \frac{dv}{dt} &= +\lambda u - kv - \frac{1}{\rho}\cdot\frac{\partial P}{\partial y}. \end{aligned}$$

Differentiate the first with reference to y, the second with reference to x and subtract; furthermore put

$$\zeta = \tfrac{1}{2}\left[\frac{\partial u}{\partial y} - \frac{\partial v}{\partial x}\right]$$

$$R = \zeta + \tfrac{1}{2}\lambda;$$

introduce the equation of continuity in the form

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} = -\frac{1}{\rho} \cdot \frac{d\rho}{dt}$$

and the equation of elasticity for gases

$$\frac{P}{\rho} = AT,$$

and there follows Marchi's fundamental equation

$$\text{(I.)} \quad \frac{1}{\rho} \cdot \frac{d\rho}{dt} = \left\{ \frac{1}{R} + \frac{k\zeta}{R} \right\} + \frac{A}{2RP} \cdot G_p \cdot G_\tau \sin \gamma .$$

In this equation: G_p is the pressure gradient, G_τ the temperature gradient, both reckoned as positive in the direction from higher to lower, and γ is the angle between these two gradients reckoned as positive when G_p lies to the right of G_τ.

This formula shows that the variation of the density at any point in the atmosphere as expressed by the first member of the equation depends upon two expressions as given in the two terms of the second member; the first of these two terms depends only on the atmospheric motions prevailing at that moment and place (i. e., on R, ζ, and k), while the second term contains also the two gradients. Marchi in his present note in the Rendiconti discusses the latter of these terms. To this end it is necessary first to establish the sign of R; this is defined as positive for the cyclones of the temperate zone but negative for those of the torrid zone.

In the present review we will consider only the first case, so that we have the following theorem: In general, where the angle γ is positive, an increase of density is taking place [in consequence of the motion of the air] and inversely.

If then we follow an isotherm that passes entirely across a cyclone, keeping the higher temperature on our right hand, we find denser air in the first part of this line but rarer air in the latter part. There is, furthermore, always a line for which $\gamma = 0$; this line separates the region of condensation from that of rarefaction: the former region is designated by Marchi as the rear of the cyclone, the region of rarefaction he calls the front of the cyclone.

Passing over Marchi's treatment of secondary formations we follow his further development. We obtain a new equation when we write the equation of elastic pressure in the form

$$\log P = \log A + \log \rho + \log T$$

and differentiate it with reference to the time so that

$$\frac{P}{1} \cdot \frac{dP}{dt} = \frac{1}{\rho} \cdot \frac{d\rho}{dt} + \frac{1}{T} \cdot \frac{dT}{dt}$$

and substitute herein the value

$$\frac{dT}{dt} = \frac{\partial T}{\partial t} - G_\tau V \cos \alpha ;$$

where V is the velocity of motion of the air, α is the angle between this motion and the temperature gradient.

If we introduce ε or the deviation of the wind from the gradient of pressure (positive when V lies to the right of G_p) so that

$$\alpha = \varepsilon + \gamma$$

and substitute all these expressions in equation (1) we obtain

$$\frac{dP}{dt} = P \left\{ \frac{1}{R} \frac{dR}{dt} + \frac{k\zeta}{R} + \frac{1}{T} \frac{\partial T}{\partial t} - \frac{G_\tau}{T} \cdot V \cos \varepsilon \right\} +$$

(5)

$$\left\{ \left[\frac{A G_p}{2R} + \frac{PV \sin \varepsilon}{T} \right] G_\tau \sin \gamma + \frac{2PG_\tau}{T} V \cos \varepsilon \sin^2 \tfrac{1}{2} \gamma \right\}$$

This equation gives again the change of pressure in the form of two terms, the second of which depends upon the angle γ.

But in order to find the true change of pressure Marchi adds still another term of the form

$$g \int_0^H \frac{d\rho}{dt} dz$$

which represents the total variation, according to the hydrostatic law, in a vertical direction from the lower plane (0) to the upper limit (H) of the cyclone. In the deduction of this term Marchi uses only the second term of equation (1), and the assumption that average values of the temperature gradient, of the pressure gradient, and of sin γ, prevail throughout the whole depth of the cyclone.

If in studying changes of pressure we consider, in general, only those terms that are affected by the distribution of temperature, we can express the collective influence of these in the following formula :

$$\pi_2 = G_\tau \Big[\Big\{ \Big[\frac{A}{2} + \frac{gH}{mT} \Big] \cdot \frac{G_p}{R} + \frac{PV \sin \epsilon}{T} \Big\} \sin \gamma + 2 \frac{PV \cos \epsilon}{T} \sin^2 \tfrac{1}{2} \gamma \Big].$$

From this formula it is evident that the cyclone is intersected by lines of no variation of pressure, for which, therefore [so far as the influence of temperature is concerned]

$$\pi_2 = 0 .$$

Since, furthermore, in one part of the cyclone the pressure steadily increases, but in the opposite portion diminishes, we can consider this as due to a movement of the whole cyclone in space. When, therefore, the distribution of temperature is represented by isotherms trending east-west, as is frequently the case in the temperate zone, having the higher temperature on the south, then there occurs an increase of pressure in the hinder or westerly part of the cyclone and a decrease of pressure in the preceding or easterly portion, or in other words, the cyclone experiences a transfer towards the east. If we consider the quotient $\frac{\pi_2}{G_p}$ as an approximate measure of this movement, then we can put the velocity of the centre of the cyclone (defined as the middle of a diameter drawn in the direction of the movement) equal to the mean of the velocities of the two ends of this diameter.

For this average Marchi obtains the expression

$$U_p = \Big[\frac{A}{2} + \frac{gH}{mT} \Big] \frac{G_\tau}{R} + \frac{PG_\tau}{G_p} V \sin \epsilon .$$

From this formula, which may be considered as the principal result of Marchi's investigations, he draws the following conclusions :

(*a*) The inner isobars move with less velocity than the outer isobars ; from a numerical example Marchi finds 10 meters per second for the velocity of transfer.

(*b*) The velocity of progress of the cyclone is proportional to the thermal gradient at its centre.

(*c*) This velocity is greater in proportion as the [absolute] temperature is lower.

(*d*) The axis of the cyclone is, in general, inclined backwards.

These conclusions to which Marchi has attained by deductive calculations agree in a remarkable manner with a series of results of observation. Therefore the query is so much the more proper as to how far these have been deduced in an unobjectionable manner. Although in this respect we ought to wait for the complete memoir to answer many queries, yet the present reviewer sees no reason to delay stating that the foundation of the analysis seems to him to be insecure. This objection refers principally to the fundamental neglect of the vertical motions of the atmosphere.

Although the vertical component is small in comparison with the horizontal motions, still it does not thence follow that its influence in the equation of continuity may be neglected. In this equation the term arising from the vertical component can be considered as at least of the same order of magnitude as that depending on the change of density which has been specially investigated by Marchi. The latter would be more than completely compensated by an extremely gentle vertical current.

Again, Marchi executes an integration throughout the whole depth of the cyclone for an expression in the denominator of which there occurs the angular velocity of the air. According to the views ordinarily accepted as to the mechanism of a cyclone, the angular velocity certainly varies and even changes its sign at great altitudes. Hence Marchi's integration relates to a function that becomes infinite between the limiting values. But such an integration is not allowable without further investigation.

In general all the formulæ above given by Marchi are affected by the unfortunate peculiarity that a quantity (R) occurs in the denominator that is very small (absolutely as well as relatively), and whose sign changes with the geographical latitude, and whose numerical computation has not been even attempted. Hence all the conclusions deduced therefrom become unsafe.

(Signed) A. O.

ARTIFICIAL RAIN EXPERIMENTS.

EDWARD POWERS, C. E. *Should the Rainfall Experiments be Continued?* Delavan, Wis., Printed for the Author, 1892.

The literature on the subject of artificial rain is increasing very rapidly, as is natural when we consider the general interest that has been taken all over the world in the experiments made in different parts of the United States in this direction. The latest addition which we have seen is a small pamphlet by Mr. Edward Powers, author of "War and the Weather." It is a criticism of Prof. Simon Newcomb's contribution to the article in the North American Review for October, 1891, entitled "Can we make it Rain?" the other part of that article having been written by Mr. R. G. Dyrenforth, who had charge of the experiments in Texas during the summer of 1891. Mr. Powers affirms that Prof. Newcomb is wrong in saying that he claims that concussion could condense enough vapor to form clouds and rain-drops, for he believes that the clouds caused by the explosions are a secondary effect produced by the mixing up of a warmer and a colder current, which unite in consequence of the expansion of the lower current. The work of the explosions, Mr. Powers says, may be compared to that of the spark that lights a conflagration; it simply starts it. The evolution of latent heat from condensing vapor, together with the existing motions of the currents, are what do the work after it is once commenced.

Two of the great objections to Mr. Powers' theory are that the rains which he claims have been produced by the firing in battles, often do not fall for hours after the firing ceases, and further, that there exists no such an arrangement of currents in the air as he calls upon in attempting to produce rain by artificial means. On these points we quote the author: "In speaking of the experiments in Texas, Prof. Newcomb asks, 'What went on during the hours that elapsed between the sound of the last bomb and the falling of the first drop of rain?' and answering the question he asserts that if the aqueous vapor in the surrounding air did not slowly condense into clouds and rain-drops in defiance of physical laws, then the hours must have been occupied by the passage of a mass of thousands of cubic feet of warm moist air, coming from some other region to which the sound could not have extended. In this he ignores the possible interference with each other of air currents flowing in opposite directions. There is no difficulty in showing that the rain which is produced by a battle may commence east, west, north, or south of the battlefield or at the battlefield itself, and that in each case precisely the same principles will be involved; and, further, that the passage of the hours between the last shot and the commencement of the rain, so far from furnishing a scientific argument against the theory, is one of its necessary factors. . . . The whole difficulty will disappear if it be recognized that there are two currents that take part in the process, that the lower of these is the equatorial, or any other humid current moving eastward, and the upper the polar or any other cold current moving westward. The logi-

cal deduction from these premises is that the mingled air should take on a circular motion, the centre of the disturbance remaining over or near the battlefield until the force of one current overcomes that of the other, and observation shows that the victory must always in the end be to the equator rial, or the current moving eastward, for a storm, when developed, moves in that direction.

"But it may be said, 'The polar current on which you depend to keep the embryo storm centre over the battlefield is hypothetical.' That may be, but too many scientists believe in it for its probable existence to be ignored in this discussion. 'But in following Maury who holds that it flows from the northeast, you ignore the better theory of Ferrel, who shows that it flows from the northwest.' I accept the theory which agrees the better with the facts to which I seek an explanation. And as to Ferrel, whose labors some affect to esteem so much more highly than those of Maury, he is not infallible, however much his methods may be admired, for the assumptions on which he founds his theories are not always true ones."

It is hardly necessary for us to add that we fail to see that Mr. Powers has made his case any stronger in this new contribution to the discussion.

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